

ANALYSIS

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ANALYSIS COMPETITION—EIGHTH ‘PROBLEM’

A report by Professor R. B. Braithwaite on the seventh ANALYSIS competition, together with any winning entries, will appear in October, 1955.

The eighth ‘problem’ is set by Mr. A. M. MacIver of Southampton, and is as follows :

“ HOW CAN I THINK IT POSSIBLE THAT I MIGHT BE MISTAKEN ? . ”

Entries (not more than 600 words), accompanied by a stamped, addressed envelope or international stamp voucher, should be sent to : **THE EDITOR OF “ ANALYSIS ” BY FRIDAY, SEPTEMBER 30th., 1955. No entries should be sent to Mr. MacIver.** Contributors may submit entries either under their own names or a pseudonym. A report, together with any winning entries, will be published, if possible, in ANALYSIS for December 1955.

THE EDITOR.

THE ORIGIN OF PAIN

By BRIAN O'SHAUGHNESSY

PAIN, some have said, is in a ‘different realm’ from the kind of thing we normally regard as its occasion. Because of this, it is not possible for there to be a genuine connection between cuts, blows, and the pain we always associate with them ; this association cannot ever provide us with a reason for expecting pain.

1. *On the idea of ‘No reason for. . ’*

(a) I toss a penny and it comes down heads five times in a row. I could reasonably wonder if the penny were loaded—and it could turn out not to be loaded. Immediately the question ‘Why were there five heads in succession?’ drops out of consideration. And it does so *entirely* (without remainder, so to speak). The reply ‘No reason!’ could now be said to approximate to a rebuke.

Suppose a penny comes heads 1000 times in a row ; after that it behaves just like any other penny ; for ever and ever. A prolonged and careful investigation brings to light nothing

that might conceivably explain 1000 heads coming in succession. I want to suggest that this past regularity is like a perpetual wound in the side of knowledge : there is nothing that can allay the trouble it causes us. We will, I expect, eventually grow to disregard it ; but it will not bear thinking on.

' But if a regularity of 5 can be wiped out without remainder, why cannot we do the same with a regularity of 1000? In what way does 5 differ from 1000, other than in size and mathematical properties, and the like? Where does the dividing line occur and how—where, in what book—do you find out where it occurs?'¹ These are questions which state a puzzle. This change, rather than the usual examples of water into steam or, worse, paper into 'not-paper' i.e. space, might be put forward as a genuine instance of "quantity into quality".

(b) These cases could prove misleading. Suppose that five pimples on someone's arm lie in a straight line. Is there any reason for that? There is, a nerve runs across their path ; this would be a reason. And this makes a reason sound as if it were some sort of natural occurrence, like a nerve that runs beneath the skin.

It is not so unlike that either—for we look beneath the skin. And there need be no reason why the pimples lie as they do. This, however, is not the same as saying : How they would lie was completely unpredictable. The appearance of each pimple might be predicted from quite separate data ; we plot their positions, and we *notice* that they lie in a straight line. That's all.

Some will seek refuge in the notion of complexity. "It is all so terribly complicated, we just haven't looked long enough" (As if we were trying to find a needle in a haystack). If it is sense to say that it is sense to deny it ("We've counted every straw five times"). And if 'We haven't looked long enough' is not so used that the justification for uttering it is that we haven't discovered any reason why ("All the same it must be here") then it makes sense to say "I've looked long enough and I can't find any reason why" and therefore "there can't be one". Contrast : looking for the cause of baldness or cancer.

This is concealed by the earlier examples. For we do not usually expect to be able to predict the outcome of a toss, cf. machines tossing pennies. It may seem that it is for this reason that the question 'Why were there five heads in succession?' drops out of consideration.

The same situation might arise if a group of machines are
 3.1. In what way do Africans differ from Europeans, other than in colour, etc.? That is to say : they are *humans*. Well, I am not interested in emphasising that ; is a number.

tossing pennies. The pennies land in a straight line, and the behaviour of the pennies and machines is predictable. But it need not be predictable : one important piece in the machine may be so fine as to resemble a piece of grass at the mercy of the breezes ; prediction would then be *out of the question*.¹ On the other hand it could be like a steam hammer.

Suppose someone points at a haphazard array of pimples and says "Is there any reason why they lie like that?" "Like what?" "Like *that*" pointing or tracing out a shape. They form a confused and intricate pattern—but it is one I happen to be interested in! I say "How on earth did you know? I drew it on my arm and it chafed the skin". But often we just could not respond to this question at all. "What shape is a haphazard array?" is similar to "How bright is the not-redness of the blue sky?"

On April 1st the genius enters his laboratory and switches on the machines ; a shower of pennies fly in the air and land in the shape APRIL FOOL. Who but someone with a *mental disease* would think 'Is there any reason for that?' This is how people react : they see what is there, feel foolish and smile, and look around for colleagues. There could be the *phenomenon* of 'double-take' e.g. seeing one's arm as someone else's arm. If the pennies formed A one might wonder if there were any reason for that, i.e. we mean to connect it with human agency, for certain (European-type) humans have a monopoly of the shape A. Ancient Egyptians had no monopoly of the profile of a bird : Nature has the original, the archetype—birds, fossils. We have a complete monopoly of the shape APRIL FOOL. Often, then, we just cannot speak of 'the shape APRIL FOOL'—for example, is A the same shape as *A*? It has yet to be settled.

We see that the question 'Is there a reason why?' is almost a question of *genre*. For if the pennies wrote '2 and 2 make' I could hardly predict that the last machine was going to throw '4', on the grounds that 2 and 2 make 4. (Surely 4 could be 5 in another notation.) Or if they said 'the later Eliot is good' I could hardly predict that the middle machine is going to write 'not so' and, by way of justification, pick up a book of his poetry and point to certain lines. *But couldn't I?* Just as we hope to exhibit a distinction at its clearest, we find it has

¹ Are accidents predictable because, for a split second, we can see what is going to happen? For 3 seconds then? Then that doesn't mean for a day : to try and predict it from the events of e.g. yesterday would be insane. (But now, I think, we are leading the word 'predict' in an artificial direction.) Are the cases in which we predict an accident the day before (gang-warfare) related to most cases *solely* as problems of great simplicity are related to problems of great complexity?

gone! Cf. Wittgenstein's ' If you say " I want the *one real Smith of flesh and blood* in my thought—not some substitute or shadow " the answer is, that this is exactly what you have got '.

2. *Different realms.*

Let me describe a case which illustrates a serious meaning for ' Because they are in " different realms " we cannot ever expect a regularity to continue '. Suppose we make the following discoveries : the number of Beethoven's symphonies is nine, the number of edges to the sodium sulphate crystal is nine ; the number of his piano concertos is five, the edges to the sodium chloride crystal are five. Similarly for Mozart and about 50 of the more important composers. Beethoven stands for Sodium, Mozart for Potassium, sulphate for symphony, chloride for piano concerto, and so on. Fifty composers and about twenty musical forms ; about a thousand instances in all.

Boccherini is Cadmium, and cyanide is string quartet. There are 24 edges to the cadmium cyanide crystal ; there are, however, apparently only 23 Boccherini string quartets! A musicologist decides to go hunting for what he calls " Boccherini's missing string quartet ". What I want to ask is : shall we say his behaviour is rational ?

Well, I cannot answer for you, that is your affair. But I would be inclined to say that it is not rational. Suppose there are 9 sides to the cadmium sulphate crystal, assume I have no idea at all as to the number of Boccherini symphonies—and my life hangs on getting it right! (Or the life of a friend). Who but someone fascinated by a theory, or a madman, or a kind of living statue, could stop himself choosing the number nine? Suppose he chose eight and it turned out to be nine, then wouldn't his friend have a right to execrate him? This situation depicts one of the boundaries of human nature—such a situation just cannot be faced.

It is conceivable that a certain emperor, obsessed with Chemistry and " Universal Harmony ", compel the musicians of his land to write in accordance with certain chemical charts that are issued to them. But this possibility has no relevance here.

Now consider this example. One day a piece of paper flutters down from the sky ; we pick it up, and we see that it has an algebraic equation written on it. We substitute for x the values 1, 2 . . . and this gives us a set of values of y which, for some reason or other, we decide shall stand for the frequencies of sounds. Let us say we are playing a game. Another slip of

paper then flutters down with another equation written on it, and taking $x = 1, 2, \dots$ we get values of y which we interpret as the duration of each corresponding sound : a crochet equals 32, a quaver equals 16. We take the first two hundred of these notes and we play them on the piano. The result is *great music!* Two hundred more and there is more great music, great arias—not just good arias, but really *world shattering* ones! And so on for as long as we like. Then these two equations would be like the goose that laid the golden eggs.

I do not think we would accept such facts as ordinary common-or-garden data : the stuff of which predictions are made. Then how would a rational being behave if he were to encounter someone plotting further values of x and y ; will he be in a state of " happy expectation "? There is no telling : he may think he is mad, he may really think he is in a dream—he could do anything! " But " it may be said " why doesn't he wait a moment and hear that aria? " We have said what would be a rational response to this situation. Or, if you like, we have said that it cannot call forth a rational response. Or, anything is a rational (or irrational) response.

If we are thinking of works of art, then there is no such thing as the goose that laid the golden eggs. When we try to fill in the picture of this goose, we have to suppose to be true, such states of affairs as would corrupt beyond recognition the character of art. Indeed, there are many apparently trifling things which would constitute an equal threat e.g. suppose we found that Himmler was a great poet, had written all the Shakesperian lyrics (" Sigh no more, ladies, . . ."). Or that Mozart looked exactly like Himmler. Almost any elbow could be Mozart's elbow, but there are many faces which it would not *make sense* to assign to him. What shows in a face, unlike what shows in an elbow, resembles the sort of thing we may see in someone's behaviour. Looking into a person's eyes is near enough to looking into his soul, but peering at his fingernail is just peering at a fingernail, a thing. Typically it is so, anyhow.¹

This sounds like language dictating to reality. Is Philosophy, then, close to " profound science "? If it is, it needs to be distinguished from what follows.

During this century, there occurred in science, changes which may appear to have done everything short of *wash away* a number of our 'famous' concepts. E.g. discoveries concerning super-ego development in infancy, and its bearing on the ordinary social

¹ Is it just sheer good luck that the human voice is such a fine musical instrument ?

behaviour of adults. Also, it may be, the curvature of space ; the electronic structure of matter ; and so on. Our concepts could hardly be said to guarantee us against this sort of thing. Anyone who thought they could was badly mistaken.

Do our concepts guarantee us against anything? What is there to say. True, it would be strange—as if, like God, one could use one's mind to vouch for reality. Cf. "It wouldn't make sense if these sufferings were *never* somehow, in another life, to be put right". This thought earns our respect, not just because it is charged with feeling nor simply because it touches our notions of justice : it could be put forward as a proof of another life and not be altogether absurd. For, oddly enough, it is not really the same as "I just refuse to believe he is dead" (a pathetic remark, because it shows the mind caving in). I think it gropes, ineffectually, around the edges of the idea we are considering.

Well, is it *true* that they guarantee us in any way? This question is overdoing it. We describe an unusual example ; that *description* is, we may say, part of the philosophical datum. We want to ask "Now if that happened, then . . . ?" "Can you imagine that happening?" and so on. To reply "I've no idea" "I wouldn't do this" "I can't imagine it" and the like. A Tower of Babel, filled with many voices on many levels ; and how could all of these voices be run together? So are we doing wrong in describing strange cases—does it set up nightmares ; or commit us to "theses" we may later wish to disown? So cannot it (cannot what) even be said?

My own opinion is that the description of strange cases is, at its best, the bringing to light of a sort of Medusa's head—something the mind cannot bear to look on, a devil it is possessed by, e.g. Wittgenstein's example of the savages who decorate their walls with the calculus. These arouse intense philosophical anxiety : they must be 'dealt with' *at once*, labelled, and buried.

Now it is not clear whether we should say that 'aria-equations' must be disregarded, or are to be seen as actually undoing concepts, or seen as undoing the whole fabric of reality—hence, talk of dreaming. Then does the whole of reality vouch, in its turn? This would seem to give it a specific nature (essence). And the description of that strange case, may be seen as an instrument with which we characterise reality.

"All our ideas are loose and separate" said Hume. This, and the idea of logically possible worlds, both assert that reality is atomic ; each offend on the same score. Reality is as it is.

Alternatively, we say " Reality is like this " and then we do something analogous to depicting, pointing, reminding.

3. Different levels.

(a) Pain is one thing and the movement of nerves is another, they are on different levels (*strata*) of being. How, then, can one result from the other?—How could Frankenstein have made the monster?

If we are equipped with many coloured points (*minima visibilia*) cf. how can we ever hope to put them together in such a way as to construct infinite space? And yet, I cannot point to a colourless hole in the sky; how would the colourlessness of glass look against this hole? But how shall I know where to place each minimum visibilium?¹

Now consider this. I close my eyes and I see a dark expanse and, it may be, a few coloured "lights". Somehow I can hardly *imagine* what it would be for this "screen" to show an ordinary lifelike scene.

Is this to say that the visual building materials in question, those coloured lights, are somehow *necessarily* coarse grained and in short supply? Or, that our success in representing scenes on surfaces (sufficient occasionally to deceive) upsets our spatial concepts—just as 'aria-equations' could upset our conceptual equilibrium? This is a sort of serious variant of the problem of error.

Well, if I am handed a few large coloured squares and asked to make a mosaic portrait of someone, then I shall be at a loss. That is so. If someone with his eyes closed announces that he sees a scene, describes it in detail, but it is not any actual scene; again I shall probably be at a loss; for circumstances are important.² Suppose we were to train our eyes fixedly onto a sunlit scene for ten hours—then suddenly close them. I do not think we would be *utterly* stupefied if, for a moment, it should seem that our eyes are open! And then we see the scene approach and dissolve into an "inner world". Cf. dense red smoke, or the blue sky: these cases seem to straddle both worlds.

This is a striking phenomenon: a case of illusion, shall we say, which is not hallucinatory—a new dimension of illusion. One is tempted to say that the difference between an hallucination and this case is a difference of *composition*: an hallucination is composed of non-existent entities, or false representatives of existent entities; the scene in question is composed of something

¹ In what way are *minima visibilia* real? Are they unchanging? Have they identity?

² What if his eyelids were artificial, plastic, and one-way transparent?

far more private, more my own—my shell, my visual sensations. Am I contacting the world or myself? Am I introspecting without knowing it? (Pinch your eyelid and say what is the spatial relation between the pain and a visual sensation. A wholly barren experiment). These last suggestions are, I think, reasons for withholding the expression ‘visual sensation’.

To say pain and the movement of nerves are in separate categories of being, does not mark it off from the following. A native rubs two pieces of wood together; at the end of a certain time they are alight. He began with “wood and movement”, he ended with wood, smoke, fire, and crackling noises. I cannot tell you the colour of the movement, but the fire is probably red. Pieces of wood have a certain weight and a temperature, but crackling noises have neither. And we say the native started the fire by rubbing two pieces of wood together, and we say he knows a way of making smoke.

Perhaps the central difficulty here should be: how can we build a bridge between a jumping nerve in a tooth, and the ache experienced by its owner? And what would it be like to find something half-way between a jumping nerve and a pain?

We can parody this question with “Yes, what *would* it be like?” This is to liken it to “We are searching for a method of constructing a closed two-sided figure” and that, perhaps, seems rather too facile an approach. But what enables us to parody it—our better understanding of logical necessity—is not trivial. Even so, I doubt if it exhausts the idea.

We discover that any position in the body is connected with a position in the brain. A necessary and sufficient condition of experiencing pain in one’s knee is that a specified change occurs in the brain in, say, the part of the body-image associated with the knee. Then, for a moment, we may imagine that here we are passing through the early stages of a journey that is going to take us from one world to another, cf. going to heaven. The brain is very nearly a metaphysical thing: as we uncover layer upon layer, we could almost suppose we are going to encounter *thoughts themselves* buried somewhere in its centre (pineal gland). Once this is said, the fact that we have tracked pain down to its lair—deep in the brain—will probably not be seen as much of an advance. Perhaps that is true. For suppose this body-image were a separate portion of the brain, and could be transplanted anywhere in the body, and still play the same role. Or simply be grafted onto the surface of the body. Or perhaps be carried around in a box.¹ So that, we shall say, in the end our search

¹ Action via connecting nerves, may conceivably become action at a distance, cf. radio.

has led us (almost) outside the body. [Then could it lead us ultimately (bedrock) to a very slight magnetic field around the head? I say this to discourage over-interpretation of the example.¹] Although it will be a step forward in knowledge, it can hardly meet our philosophical needs. Supposing the self is thought of as "one" and "internal" in nature; I wish to point out how the brain need not duplicate these features.

(b) It seems we must be left with a problem of levels. For what could be a "uni-level" origin of pain? Might it owe its existence to some other item of consciousness? For example, may one pain produce another? A pain can gradually alter in character; a sensation of one kind may change, slowly, into a pain. We do not, however, regard the earlier as producing the later; even if these changes were to be customary.

I am reminded of Berkeley's "All our ideas are visibly inactive, there is nothing of power or agency in them". The "proper objects of sight" were "lights and colours". Do we ascribe causal power to colours? Mixing red and yellow paint equally produces orange paint, and that is no surprise; it seems almost to be the materialisation of an entailment (ectoplasm?) And yellow mixed with blue gives green? That is something we learn; nevertheless it is a colour reaction, as opposed to a chemical reaction e.g. phenolphthalein (colourless) + any alkali → a red liquid. The colours of the rainbow mixed together make white; that was discovered by Newton.

These are examples of mixing and its results; neither colour is the active agent. Yellow, green, and blue, are a trio of separate natures in a way in which red, orange, and yellow are not. If we ask why yellow and blue make green, it is not going to be easy to give an answer. Is it like 'yellow and blue make red'? Could yellow and blue have made red rather than green? Then is it an empirical fact, a strange almost solitary one of its kind, that the quasi-logical relation 'yellow + red → orange' has been left in the world? Or is it simply a coincidence i.e. it is odd that it should match our thought. How do we settle this?

A wine glass may be shattered by a sound of very high frequency; also by a hammer. This could seem very puzzling. A noise may frighten or startle someone, but how could a wine glass be aware that a sound had come into existence? And what could it do supposing it were to know?

A sound, surely, is something impalpable: I cannot select any position in mid-air and say "No doubt the sound of my voice

¹ Do we turn back towards the body, the flesh, as a salmon in spring will head up-stream?

is here" as if it were some sort of gas—like air—that emerged from my mouth. ("Either the sound is where it issues from" so it seems "or it is in the minds of its hearers"). But it cannot be where it issues from, for then we should be unable to hear it. This view would put talking on a par with telepathy.)

In all these cases we turn towards a 'substratum', usually material: nervous connections, etc.

(c) That is an acorn, but it is also radioactive! This is a toothache and intense; what else is there to this intense toothache? What lies on its other side? cf. the moon. We want to burrow under its surface; but what direction are we to take and when are we there? Pain is 'one-dimensional', it wears its heart upon its sleeve.

Something about a present pain can make one say "Since this is utterly separate, it owes its existence to nothing, and vice-versa". Self-sufficient, complete, isolated, powerless. 'The same pain' has a shaky use; some criteria come naturally e.g. identity of origin. "So it never recurs, so pains are loose and separate"? It doesn't follow.

One would need to see a pain actually give birth to another, before we would say it made that pain. What would it be to encounter pain in the *very act* of forming (the universe preparing to move over for it)? Cf. an embryo, an explosion, a sneeze. These difficulties also apply to movement, colour, etc.

"What is this pain (redness) composed of?" is absurd. Either we say it is composed of itself, its very own substance—and nothing else can be—or we reject the question. 'Red+Blue→Purple'; it doesn't follow that red has more reality than purple, that purple is a kind of logical shadow of red. For 'Purple—Blue→Red' e.g. running a magnet through a fine mixture of blue iron filings and red sand. There is nothing analogous with kinds of pains.

What are the parts of a uniform area of colour? There are no rules, if a group of people agreed we wouldn't understand them. We cannot say: this area of colour (pain) ceases to exist because its parts are no longer related in the same way. A pattern of colours can just fade away.

Suppose someone said: let us try to do for pain what science has done for nitric acid, or heat, let us investigate the deeper recesses of its nature, cf. elements, molecules, quanta. We wouldn't know what he was thinking of. For what particular properties has it got—nitric acid attacks copper—whereby its nature could express itself?

A NOTE ON THE BODY-MIND PROBLEM

Reply to Professor Wilfrid Sellars

By KARL R. POPPER

I AM very grateful to Professor Wilfrid Sellars for bringing¹ my paper "Language and the Body-Mind Problem",² to the attention of philosophers, and even more for his kindness in describing it as "challenging", and as "telling, if uneven". Of its unevenness nobody can be more aware than I. I think I am more sensitive to it than Andersen's princess was to the pea. And although I count its less than seven pages among my scanty laurel leaves, I could not rest on them even if I wished to. But the small hard peas which bother me and keep me awake at night seem to have been well hidden, and in a spot far removed from Professor Sellars' two largish lumps of stuffing which, I believe, are not at all hard to smooth out.

(1) As to the first lump, Professor Sellars, after quoting me correctly at some length, proceeds to 'focus attention', as he puts it, 'on the statement [Popper's statement] quoted above, that "... if the two languages are not translatable, they deal with different sets of facts".' And Professor Sellars then goes on to say that a "fact" may be either a "*descriptive fact*" or else something like 'the "fact" that we ought to fulfil our undertakings', which I may be permitted to call a "*quasi fact*". And he says that my argument would be valid only if it would contain "the premise that both languages in question *have the business of describing*", i.e. of stating "*descriptive facts*".

Now I agree with every word of this but I completely fail to see its relevance: in focussing attention upon one statement, Professor Sellars got, understandably enough, its context out of focus.

For (a) the premise which, according to Professor Sellars, makes my argument valid, was clearly enough indicated in my own argument which therefore is itself valid, according to Professor Sellars. Moreover, my argument has the form of a *reductio ad absurdum* of the "two language theory", and the premise correctly demanded by Professor Sellars is not mine but part of that theory. It is, indeed, presented in my argument as part of the "two language solution"—of "the view that . . . the

¹ By way of his "A Note on Popper's Argument for Dualism," *ANALYSIS*, 15, pp. 23f.

² Sic (*not* "Mind-body problem" as Professor Sellars writes). See Proc. of the 11th International Congress of Philosophy, Brussels, 1953, vol. VII, pp. 101–107.

statements of physics and of psychology are . . . two ways of talking about the same facts" (which clearly indicates that these "facts" are "descriptive facts" in Professor Sellars' terminology). My own contribution consisted, simply, in pointing out that, once the two languages (of physics and of psychology) are admitted not to be translatable into each other, they cannot be said any longer to talk about the same facts, and must be admitted to talk about different facts—where "facts" mean whatever the two-language theorists meant when they said that physics and psychology talked about the same facts.

Thus the problem of "quasi facts" simply does not arise.

All this can be verified by reading more closely the passage from my paper which Professor Sellars himself quotes at the beginning of his paper: it is the passage which gets out of focus once he focusses attention on part of it. (There is a not very important misquotation—"set" instead of "kind"—in the focussed passage.)

So no hard core, no difference of opinion as far as I can see, underlies Professor Sellars' first lump—although I seem to differ with him about the relevance of his comments.

(2) Now to smooth out the second lump. "In the later sections of his paper", Professor Sellars writes, "Professor Popper makes a telling, if uneven, defence of the thesis that *aboutness* or *reference* cannot be defined in Behaviourense." (Professor Sellars himself believes in the truth of this alleged thesis of mine.) I must confess that I was very surprised to read this. I am not aware of having ever tried to defend anything of the kind. It happens to be one of my oldest convictions that a thesis of the kind here attributed to me—that such and such *cannot be defined* in somebody's language—is *nearly always irrelevant*. (It is not irrelevant, of course, if the opponent's thesis was one about definability. Definability may be interesting in certain contexts, but to say a term is not definable does never imply that it cannot be legitimately used; for it may be legitimately used as an undefined term.) There was no need for me to read through my paper in order to be sure that I never maintained anything like the "thesis" attributed to me by Professor Sellars. But to make doubly sure, I did read through my paper, and I found no trace of such a thesis on definability. And to make trebly sure, I herewith publicly recant any theory I may ever have advanced, based upon the thesis attributed to me by Professor Sellars: not because the thesis is false (I agree with Professor Sellars that it is true, and I even agree that my arguments might be used to support its truth—which may perhaps

explain the misunderstanding) but because I should hate the idea of philosophising with the help of arguments about non-definability.

Professor Sellars goes on to say "And he [Popper] is surely right [in holding the thesis I have just repudiated]. However, at this stage he [Popper] tacitly adds the premise ‘‘*E* is about *x*’ is a descriptive assertion”.

It is hard for me to check whether or not I have added this premise tacitly at this stage, since “this stage” is not indicated by Professor Sellars—or only indicated with the help of a reference to that alleged thesis of mine which I fail to find anywhere in my paper. (I may here warn readers that seven of the passages in quotation marks in this second part of Professor Sellars’ paper are not quotations from my paper, as some might think. Two others, “Name relation” and “Causal-physicalistic”, did occur in my paper, but the former hyphenated, the latter unhyphenated.)

If, however, I have somewhere “tacitly” and unconsciously added the premise which Professor Sellars’ says I have added (I cannot detect any trace of it) then I wish, again, to recant. For I am in complete agreement with Professor Sellars’ thesis that if a statement *A* says that another statement *E* is about something, then *A* usually does not, to use Professor Sellars’ words, play “the same sort of rôle as ‘The Moon is round’”. *A* need not be, and usually is not, “descriptive” in the same sense as the statement about the moon (although it may be: “What was your last lecture about?”—“It was a lecture about probability”, is an instance of descriptive usage).

I also agree entirely with Professor Sellars’ concluding remark that ‘from the fact, and it is a fact, that what Professor Popper calls the “name relation” (paragraph 5, ff) is not definable in “casual-physicalistic” terms, we cannot conclude to the truth of Dualism.’ Exactly. This is why I never said anything about definability. Indeed, had I no stronger arguments in favour of my dualistic faith than this completely irrelevant fact (for I agree that it is a fact, though completely irrelevant), then I should be ready—nay, most anxious—to give up dualism. As it happens, my arguments were quite different. They were about¹ the possible scope of deductive physical theories rather than about definability; and my thesis was that “no causal physical theory of the descriptive and argumentative functions of language is possible”.

I wish to make it perfectly clear that I have no objection

¹ This is another instance of an about-statement *A* which describes an argument *E*.

whatever to Professor Sellars' thesis—that a statement such as "*E* is about *x*" is (ordinarily, or frequently) "a device whereby we convey to the hearer how a mentioned expression is used, by using an equivalent expression". Nor do I deny that this thesis of Professor Sellars' is relevant to my own thesis. All I wish to say, under (2), is that my thesis is not based on the argument about definability which Professor Sellars ascribes to me. If it were, I should retract it.

(3) There is a remark on Professor Ryle's views in Professor Sellars' paper with which I cannot agree. Professor Sellars writes: 'I also agree that "the idea of a mutual translatability" of . . . mind talk and behaviour talk "had to be given up long since", in spite of Ryle's valiant efforts to the contrary.'

To this I should like to say that I am not aware of the fact that Professor Ryle has ever held what I call "the two languages theory". How could he, believing as he does that the problem arises out of category-mistakes within the one natural language? It is not to him I was alluding in that place.

At the same time, it is perfectly true that I had Professor Ryle in mind when, in another paragraph of my paper, I tried to show briefly that the theory of "category-mistakes" is also untenable.

If I might here add to my arguments another, then I should say this. Assuming, that by the usages of our language, expressions naming physical states are put in a category different from that in which expressions naming mental states are put, I should be inclined to see in this fact an indication, or a suggestion (not more than this, to be sure), that these two categories of expression name entities which are *ontologically* different—or in other words, that they are *different kinds of entities*. Thus I should be inclined (not more than this) to entertain the opposite conclusion from the one drawn by Professor Ryle although the premisses would be, admittedly, insufficient.

However, I am not prepared to grant the truth of this assumption quite apart from my (and from Professor Smart's¹) objections to arguments based upon the idea of category-mistakes. I find very many of Professor Ryle's analyses most illuminating, but I can only say that ordinary English very often treats mental states and physical states on a par with each other; not only where it speaks of a "mental disease", of a "hospital for the mentally sick" or of a man who is "both physically and mentally well balanced", etc. (these cases might be dismissed as deriving from

¹ See his excellent brief "Note on Categories" in the *British Journal for the Philosophy of Science*, 4, pp. 227f.)

a philosophical dualism) but especially where we say : "Thinking of sleep always helps me to fall asleep" or "Reading Mr. Smith's novels always helps me to fall asleep" (which does not mean "training my eyes on one of Mr. Smith's novels always helps me to fall asleep" and yet is completely analogous to "taking bromide always helps me to fall asleep"). There are countless similar examples. They certainly do not establish that ordinary English words describing mental states and physical states *always* belong to the same "category" (Professor Ryle has succeeded in showing that they don't). But my examples establish, I think, that the words are often used in ways which are strikingly alike. The ambivalence of the language-situation may be illustrated by an example of Professor Ryle's.¹ He says, rightly, that a child who has just watched all the battalions, batteries, and squadrons, marching past, which constitute a division makes a mistake (in the sense that he has not quite got the meaning of the words) when he then asks "And when will the division come?"—"He would", Professor Ryle says, "be shown his mistake by being told that in watching the battalions, batteries and squadrons marching past he had been watching the division marching past. The march-past was not a parade of battalions . . . and a division, it was a parade of the battalions . . . of a division." This is absolutely true. But are there no contexts in which divisions and battalions are treated on a par with each other, in perfectly good English usage? Could there not be a parade of, say, one division and three battalions and two batteries? I can imagine that this might be an outrage to military usage. But would it be an outrage to ordinary English usage?

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MISCONCEPTIONS OF INFERENCE

By D. G. BROWN

EVERYONE sees that an inference is a different sort of thing from a proposition. In logic, in particular, we have occasion to insist on the difference, in order to make clear the fields of application of 'valid' 'fallacious' 'true' and 'untrue', and the various relations between the validity of inferences and

¹ *The Concept of Mind*, p. 16f. The example of the Colleges and the University is precisely analogous.

the truth of various propositions. But logicians are tempted, on their second page, to put such points, or to explain them, by saying that an inference is not a proposition but a something else, say an operation with propositions, or a passage from one proposition to another. In so committing themselves, they usually fall in with conceptions of inference which are natural in any case, and which are then reinforced through appearing necessary to the work of the specialists in inference, logicians.

Little harm is done in formal logic by perfunctory accounts of inference. But when we are concerned not with logic, but with the nature of logic ; not with inferences but with the nature of inference ; not with the inferences people might validly draw but with what it is for people to draw them ; then we need, as in logic we do not need, to consider head-on what sort of thing inference may be. For example, is an inference, in one sense or another of the word, a process, or an act, or a state of mind, or an achievement, or a logical entity, or an event, or none of these? What we are to say to such questions is interesting not merely in its own right, but also because our conception of inference affects our conception of principles of inference.

There are several natural, and indeed serviceable, ways of conceiving inference, each of which can be more or less misleading. The object of this paper is to show in what respects they can be misleading, and thereby both to show the need for an alternative account of inference and to clear the way for it.

When crystallized into propositions, these ideas look crude enough. They are nonetheless influential, and worth bringing to the surface, and I think I attack them with appropriately blunt instruments.

Some natural misconceptions

There are two notions which in several variations and combinations contribute to these misconceptions of inference, so that I can divide my attack into two corresponding parts. These notions are (*a*) that of motion from one place to another, and (*b*) that of action. Consider some examples : Under (*a*), one who infers is thought to pass, or to be carried along, from premisses to conclusion. Or, under (*b*), he is thought to accept, or to say, something permitted by a rule which he takes advantage of. If, on the latter view, the action is done on something else, he may be thought to operate with statements or to draw out a conclusion. The two notions blend in that of a journey : one takes a journey oneself, in this case either by plodding the whole way from premisses to conclusion or by stepping on to a train that

has no emergency cord and does not stop even at Reading. A more curious blend still is inference as a step : one takes a step, and in it one is in motion, though one is at no time at the position of the rear foot, nor at that of the front, nor half-way between, but at the position which includes all of these. All these models for inference represent it as involving motion or action or both. We may dispose of them all by showing first that an inference is not a motion (or for that matter the end of a motion), and then that an inference is not an action, (or for that matter an achievement). The need for such a house-cleaning was hinted at in a comment by Mr. Richard Robinson on Cook Wilson's idea of inference.¹

Inference as motion from one place to another

We must notice first that the idea of inference as moving from one place to another does reflect a striking feature of inference. For inference seems to be of a double nature, in that where someone infers something we can distinguish what, in his view, is supported, and what supports. Further, the relation of supporting is one-way, i.e., as we might say, *from 'p' to 'q'*. Nevertheless, it is obvious that the model will not do.

To show that it will not, it is enough to remark that what moves, moves through in-between positions, at some speed or other, and takes time for the whole journey. But it makes no sense to speak of someone as stopped a third of the way from the premisses to the conclusion of his inference, or of his passing slowly from one to the other. We could say that someone was "quick to infer", or perhaps "inferred too quickly", that his wife was having an affair. That, however, would mean that it was not long before he inferred rather than not long during his inferring. Compare with it : "It did not take him long to remember" or "Before long he thought that—". Wondering or investigating can take time, and be broken off part way, but not inferring. We need not labour the point.

There is however a variant of this view, or at least a natural refuge from its breakdown, which we must consider next. Perhaps inference does not take time, not because no journey comes into the matter at all, but rather because the inference is the completion of the journey. Inference is not travelling, simply because it is arriving.² This conception is a further misconception, but it is one which reflects, imperfectly, some further actual features of inference. Let us consider both what there is in it and what is wrong with it.

¹ *The Province of Logic*, p. 87.

² Cp. Ryle, *The Concept of Mind*, pp. 299 ff.

In the shift from travelling to arriving, the journey has ceased to be the journey from ' p ' to ' q ', and has become instead a journey from an unspecified starting place to the inference, from the fact that p , that q . In the special case that one begins from knowledge that p , and labours towards the consequences of ' p ', the journey can be called one from ' p ' to ' q '; but that is inessential. Now through what territory can this journey lie? I think the only plausible suggestion is that it lies through the investigation or enquiry in the course of which one comes to infer that q . Clearly, then, the entrance of this conception is important. It directs our attention to an important and obscure relation, the relation between such things as investigating, inquiring, asking oneself, and considering, on the one hand, and on the other hand, such things as believing, doubting, knowing, and inferring. Just as it may be through investigation that one comes to believe, or through asking oneself that one comes to doubt, or through enquiry that one comes to know, so it may be through investigating, enquiring, or asking oneself that one comes to infer. So much this conception of inference succeeds in reminding us of.

It is nevertheless seriously misleading. In the first place, it tends to confuse the relation of implication with the course of an investigation. For, being parasitic on the travel-conception, it seems to derive its plausibility from the same features of inference, namely from the presence of that which supports and that which is supported and from the one-way direction of the relation of support. But in fact it derives its plausibility not from these at all but from features of investigation, especially persistence through time, which have no more to do with enquiries that lead to inference than with enquiries that lead to doubt or to knowledge.

In the second place, where something starts from one position and travels to another, a path can be traced from the first position to the second, and to set out along this path is, as a matter of logic, to approach the second position. But through such activities as investigating, enquiring, and considering there can be traced no route such that to set out and persevere on this route is necessarily to approach any outcome at all, much less any particular outcome.

In the third place, it is simply not always the case that when someone has inferred something, that is the outcome or end point of an enquiry. One may infer straight off, at once, without giving the matter a moment's thought, and often without its being desirable or proper to do so. In all such cases, the arrival

would be an arrival from nowhere, and after no travelling, and so not an arrival at all.

Inference as action

My second undertaking is to discredit that conception of inference which might be stated, in several variants, by saying that an inference is an action, or an act, or a performance, or that inferring is an activity. This view of inference has in fact been put forward as part of a view of rules of inference, from which it derives extra interest. We also have thereby further variants of the conception, to the effect that inferences are applications of rules, or that in inferring one observes a rule or conforms to a rule. For actions, performances, and the like are thought to be the only sort of thing in which one may apply, observe, or conform to a rule.

I think the main fact about inference which this view of it catches sight of is that inferences can be criticized as correct, legitimate, warranted, or not, by reference in part to whether they are or are not in accordance with principles of inference.

This conception of inference can be brought before us by some passage in which it is fairly explicit :

In "Why are the calculi of logic or arithmetic applicable to reality?"¹ Prof. Ryle says : ". . . some people have worried themselves by speculating how or why the rules of inference apply to the world ; they have tried to imagine what an illogical world would be like. But the puzzle is an unreal one. We know already what an illogical man is like ; he is the sort of man who commits fallacies [&c.]. . . The reason why we cannot imagine what an illogical world would be like is that a tendency to flout performance rules can be attributed only to performers. . . ." And later : ". . . it is nonsense to ask how the rules of logic apply to the world. Both the Procrustean and canonical rules of logic are performance rules. Only performance can be or fail to be in accordance with them. If they are applied, that is a fact about the efficiency and intelligence of theorists, not a fact about any radical docility of the world".

These views are reflected larger than life in "Reason and Conduct", by J. J. C. Smart,² where Smart considers whether an action can be reasonable or unreasonable in a sense justifying the dictum that reason can be practical. In the course of his argument he says (p. 213) : "An obvious reflection will indicate the way towards a solution of our problem. It is simply this : that theory

¹ P.A.S.S.V., 1946, pp. 22-3.

² *Philosophy*, 1950.

is a branch of practice and not an alternative to it. All theorizing is an activity. To draw a mathematical consequence is no less a procedure than is to draw the joint out of the oven. . . . Theory is drawing conclusions, weighing evidence, asserting, arguing, . . . wondering. . . ." Later he says : " ' Reasonable action ' now seems to mean ' action in accordance with the rules of morals ', just as ' reasonable inference ', means ' inference in accordance with the rules of logic '." He had spoken earlier of " a rule of inference or injunction to proceed from premisses of a certain sort to a conclusion of a certain sort." (p. 211).

These two passages are insistent enough to be taken at their word, where one might not hold Mill to account for his phrasing of this point of view : " The results of many observations and inferences, and instructions for making innumerable inferences in unforeseen cases, are compressed into one short sentence."¹

R. M. Eaton says² : " Implication is a relation between propositions . . . inference is an *act of thought* which detaches a proposition from an implication."

I forbear to quote from symbolic logicians since they may be talking about calculations rather than inference, whichever word they use. Similarly, the terminology of the mental act theorists may be deceptive, and is certainly compatible with either making or not making inference into a kind of " judgment ". Only some of them, like Johnson, emphasize the ideas of activity, purpose and free-will ; he says³ : " Logic formulates standards or imperatives which as such have no significance except as imposed on mental acts ".

The clearest indication that this conception of inference will not do is that inference lacks a characteristic essential to performances and the like. A performance must be something one could in principle refuse to carry on with ; an activity, something one could decline to engage in on some occasion. For there to be action, there must be something one could decide not to do. But,—to consider a case,—suppose that, from the fact that a patient failed an appointment, a doctor inferred that the patient lacked confidence in the treatment. Then his inference was the inference :—that the patient lacked confidence ; that was what he thought to be the case, and his reason for thinking it was the one given. Now, in such a case we can say that, whether his inference is legitimate or not, it is what it is, and he places just

¹ *Logic*, II, III, 3, 8th Ed. p. 213.

² *General Logic*, pp. 39–40.

³ *Logic*, I, p. 8.

so much reliance on it. It is not for him to infer something else, or to increase his assurance, or to infer nothing at all. It will probably be his business to look into the matter again, in case he should be led thereby into doubt about this inference. But if he says "I shall probably change my mind about this tomorrow", he is not warning us of the changeableness of his resolution, but predicting the outcome of an enquiry.

When I know that p , from which it in fact follows that q , and I consider and understand the suggestion that q , either I see it follows or I do not. I may have to do something, namely consider, ask myself, think it over. But once I do that, if not before, the inferring is either past help or past hoping for. I find that I infer or that I do not.

This irrelevance of such ideas as refusal and decision is connected with the fact that with a few exceptions the imperative of the verb 'infer' has no unparadoxical use. You can tell me what to think about, or tell me to "think it over", but you cannot tell me what to think, what to doubt, or what to infer. The nearest one can come is "Infer what you can from this information", a request which is fully complied with by sitting down and considering the question of what the information shows. But certainly the command, request, or suggestion "Infer that q " is always either consciously paradoxical or nonsense.

"Rules of inference"

It follows from these two points that to infer cannot be to conform to a rule in the most familiar senses of 'rule'. In one of these senses, rules can be laid down; e.g. a rule that undergraduates are to wear gowns at night. One can obey, comply with, observe, or simply conform to such a rule, or one can refuse. In another sense, one can take it as a rule of conduct, make it a rule, never to wear a gown, and one can adhere to or depart from one's own rule. Such rules may either require, say, the wearing of gowns, as in these examples, or permit the wearing of gowns. The formulations of such rules contain a word, often in the infinitive, for some course of action, procedure, activity or the like, in fact for the particular one involved in conforming to the rule. Inferring is never so involved.

Thus, although one can act in defiance of a rule which requires one not to, or can act consciously lacking a rule which permits one to; one's inference could not defy a rule of inference, or claim to be permitted by no rule. A fallacy in inference cannot be wittingly committed.

These denials oblige me to deal with what some people have unhappily called "rules of inference". Take : "Things equal to the same thing are equal to one another", or "If A and B both equal C, A equals B". Insofar as one could speak at all here of inference, one would have to grant that inferences could be in accordance with this "rule". (Unfortunately there are reasons not relevant to the present issue for denying that there could be an *inference* involving entailment. Let us however suppose for the moment that there is such an inference. Then it will indeed be in accordance with the principle stated.) But this typical "rule of inference" is, in pompous terms, a principle, but it is not a rule in the sense above. It is true ; and it talks about A and B being equal to C. If it were to be called a rule at all, it could only be in a sense analogous to that in which an empirical law is a rule. For example, von Frisch says¹ : "In 62 series of experiments of this type, I found that the following rules seemed to govern the response of the bees. . . . If I turned the sheet . . . so that the plane of vibration of the polarized light was shifted, the dances deviated in the same direction".

Such rules "govern" phenomena, which they mention and about which they give information. Since the difficulty that arose over an analytic principle like Euclid's first axiom does not arise with such empirical principles, and inferences about bees might well be in accordance with them, some excuse is provided in their case for the phrase 'rules of inference'. But the excuse is thin, and I suggest that the phrase "rule of inference" be dropped altogether.

The application of a principle

I have been arguing that principles of inference are not rules either conformed to or taken advantage of in inferring. The point can be put in terms of the words 'apply' and 'application', the complexity of whose use is underestimated. There is a sense in which both inferences and arguments are applications of principles. But it does not follow that they are so in that someone applies the principles in conforming to them. Certainly a man who had adopted the rule of giving equal pay for equal work could decide not to apply the rule in one of his shops, or regret his application of it there, or narrow the application of his rule ; and here the application of the rule either is, or is determined by, its being applied by somebody who conforms to it. But it is not so in other cases, whether for rules or for principles. Even a rule laid down, like the one about gowns,

¹ *Bees, their vision, etc.* Cornell U.P., 1950, p. 92.

though it is conformed to, is not thereby applied. Rather it applies to certain people, in certain circumstances, which is to say that there is question of their conforming ; they come within its application. With such rules, as with empirical laws, what they apply to is what they talk about. They have an application ; an application is neither given them nor made of them.

Coming to principles of inference, inferences are applications of them in a sense which has been explained by Ryle.¹ Not only does it not follow that they are so in that someone who infers the principle is conforming to it ; it is not in fact the case. They are so because part of what is meant by the "principle of inference" is a statement so related to the inference that it is true when all such inferences are legitimate—and because this peculiar relation can be explained in terms of the statement applying to the inference and the inference thereby being an "application" in Ryle's sense. That sense is distinct from the others I have mentioned.

Inference as linguistic transformation

The conception of inference as an operation in accordance with rules has one special form which, on account of its influence at present, deserves separate mention. That is the view that inference is an operation in which linguistic expressions are transformed or replaced by the user of the expressions, in accordance with rules for their transformation or replacement. If it were no more than a special case of the misconception under attack, it might be left to the general refutation already given ; but it has two independent sources of plausibility.

One of these sources is the so-called linguistic theory of analytic argument ; and the other is the fact that there are operations with expressions by means of which we can acquire information, viz. those operations we call calculation. Both ideas are likely to be at work when one compares inference to the operations of various logical calculi, if indeed one does not confusedly identify the two kinds of thing. In respect of both of them, the inevitable influence of symbolic logic on one's conception of inference needs to be made conscious and open to criticism.

As to the linguistic theory, I should be willing to argue that as commonly stated it was unsound, and that consequently to regard even analytic argument (let alone inference) as linguistic transformation was nearly as misguided as to regard inductive argument as linguistic transformation. As to calculation, I would sharply distinguish inference from calculation of any kind, and

¹ In "If, so, and Because," *Philosophical Analysis*, ed. Black.

in particular from those operations for which symbolic logicians have seen fit to use, as a term of art, the name ' inference '. Unfortunately both subjects, the merits of the linguistic theory and the nature of calculation, are too big to enter upon here. Accordingly the conception of inference as linguistic transformation must go unexamined.

Inference as achievement

It remains, as unfinished business, to show that ' infer ' is not an achievement verb. According to Ryle : First, except for purely lucky achievements, an achievement is the result or culmination of certain acts, operations, etc.¹ Second, " In applying an achievement verb [e.g. ' cure '] we are asserting that some state of affairs obtains [the health of the patient] over and above that which consists in the performance, if any, of the subservient task activity [the treatment] " (p. 150). I take it he implies, third, that the *subservient* activity is so in that it is directed towards the achievement ; in treating, one is trying to cure, in running, one is trying to win, and so on.

So long as we interpret these specifications as I think they were intended, ' infer ' does not meet any of them. First, as I have argued before, no enquiry, or other activity, need lead up to an inference, for the inference to be an ordinary inference, and not a purely lucky one. Second, on any reasonable interpretation, there is no " state of affairs asserted to obtain ", corresponding to the patient's being well again. One has still inferred that *q*, even when it is not the case that *q*, and ' *q*' does not follow from what one takes it to follow from. Nor does it help to include failures under achievements, for whether one is mistaken or not is simply irrelevant to whether one infers or not. Of course, there is a state of affairs asserted to obtain when one says someone inferred, either correctly or incorrectly, that *q*—a state of affairs describable by saying that, unless he has forgotten or changed his mind, he infers that *q*. That is important for other purposes, but does not make ' infer ' an achievement verb. Third, in the only " subservient activities " one could suggest, like enquiring, or considering, one is not trying to infer, but to discover or come to know. Inference is mere inference, and " He inferred ", like " He thought ", is incompatible with " He discovered " and with " He came to know ". There is written into ' infer ' the notion of a claim (to have good reasons) qualified by a disclaimer of achievement (in the enterprise of discovering).

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¹ *The Concept of Mind*, p. 151.

